for the mere necessities of calculation to act. All his argument, if I understand it aright, depends upon the displacement being by fits and starts. Thus he says (NATURE, July 28, 1870), "The precessional force has its full effect in producing the precession of the solid crust, the fluid *not having time** to diminish that effect before the axis has assumed a new position;" and "The friction of the fluid within, which *has not time* to influence the nutation before the nutation is actually produced;" and (NATURE, May 11, 1871), "Suppose a succession of slight horizontal pushes to be given to the poles in a continually altering direction, the effect will be that the revolving crust will be continually slipping over the revolving fluid, which *has not time* to acquire the new motions given *instantaneously* to the solid crust."

The leading idea in all these passages seems to me to be that the attractions of the sun and moon, to which precession and nutation are owing, act by impulses, by a succession of sharp pulls quickly repeated. This is truly enough the supposition with which mathematical calculation starts; but the real action, I need not say, is a steady, continuous, though ever varying, pull, and it is the result of such an action which our calculations in the end lead us to, by a method which enables us to get rid of the error necessarily involved in the approximate result which would follow from our first supposition.

I cannot then help thinking that even Archdeacon Pratt has for once been carried away by the beauty of mathematical analysis, and has for the moment forgotten that the conditions which it is obliged to employ for its ends do not in their initial form represent the actual conditions of nature. The explanation occurred to me on first reading his paper in the *Philosophical Magazine*, but seemed to me so unlikely that I shrank from putting it forward. I can, however, in no other way imagine how he can have come to the startling conclusion, that, if a solid shell be moved by a steady, continuous pull over an interior ball of fluid, it can make no difference in the result, whether there is or is not friction between the interior of the shell and the surface of the fluid. Archdeacon Pratt, will, I know, if I am wrong, pardon my presumption and put me right.

Barnsley, May 12 A. H. GREEN

### Pangenesis: Graft-Hybrids

EACH person who assails this unfortunate "provisional hypothesis" makes the attack from his own particular point of view. Thus, in NATURE of last week Prof. L. S. Beale, as a microscopist, objects to it because the gemmules cannot be made evident to the senses. From this somewhat narrow view of the case the atomic theory of chemistry, the undulatory theory of light, or the mechanical theory of heat, must all break down, for no one has as yet seen an ultimate atom, or an ethereal undulation. Mr. A. C. Ranyard, in the same paper, publishes a letter which is quite at variance with fact, for if he will turn to pp. 390, 391, 394, 397 in vol. i. and pp. 364 and 365 vol. ii. of Mr. Darwin's work on "The Variation of Animals and Plants under Domestication," he will there find many cases given of the scion affecting the stock and producing intermediate forms known as "graft-hybrids." Pangenesis has not yet "received its death blow."

R. MELDOLA

May 13

IN your last number Mr. Ranyard brings forward an objection to Mr. Darwin's theory of Pangenesis on the ground that the grafting of a bud on a stock of a different species does not produce a hybrid offspring. I am not about to defend the doctrine of Pangenesis, which appears to me incapable alike of proof and of disproof. It is, however, a well-known fact that the stock does affect the scion, and *vice versâ*. In Prof. Henfrey's "Elementary Course of Botany" (Dr. Masters's edition) he says, "A certain amount of physiological influence of the stock over the scion is shown to exist by such facts of horticultural experience as that the fruit of the pear is smaller and more highly coloured when 'worked on' the quince and medlar than when grafted on pear-stocks, and is earlier when worked on the mountain-ash." The well-known instances of the communication of variegation from the scion to the stock in *Abutilon*, recorded by Prof. Morren and others, are considered cases of contagious disease; but what is the theory of contagion but that the blood or other "fluid" of an animal or plant is affected by emanations, call them "gemmules" or what you will, from another individual? The same writer records an instance which he considers well authenticated of the production of the hybrid *Cytisus Adami* by the grafting of *C. purpureus* on *C. laburnum*.

ALFRED W. BENNETT

### The Rev. Mr. Highton and Thermodynamics

YOU are cruelly kind to Mr. Highton in giving him space to develop his absurdities.

His new remarks on Joule, like his earlier ones on a paper by Sir W. Thomson, simply show that *he does not understand* what he ventures to criticise. Of course, what Joule now says is precisely what he said a quarter of a century ago, with the simple difference that it is put in a somewhat more popular form.

No one who has taken the trouble to understand the experimental *facts* and the elementary reasoning of which the Laws of Thermodynamics are the condensed expression, has any more doubt of their truth than of the truth of Newton's Laws of Motion. They are, perhaps, a little harder to understand; but the proof is of the same nature, and already almost of the same extent, in the newer science as in the older one.

I have not seen the *Review of Popular Science* referred to by Mr. Highton, but I hope (for the credit of that journal) that he misconceives its statements as he does those of Joule.

Your "first reviewer" (or rather précis-writer) of his article, whoever he may be, certainly gives him no encouragement in the number for Jan. 19, whatever may have been the effect of my treatment of his not singular case.

YOUR REVIEWER

### On the Radial Appearance of the Corona

WOULD an indefinitely extending solar atmosphere, if its existence could be proved, be in itself sufficient to explain the appearance of the solar corona? Should we not still have to explain the apparent radiation which is so distinctly part of the phenomenon?—If the light or heat of the sun which radiates symmetrically outwards as from a point at its centre be the cause of the illumination, surely the figure of the corona would bear some relation to the figure of the atmosphere or medium on which the light or heat acts? Yet I think I may say that it is quite impossible to conceive a medium so distributed and arranged as to form rays such as those seen in the corona. If the recent photographs had not shown beyond a doubt that this irregular radiating appearance belongs to the corona and the neighbourhood of the sun,* it would have gone a long way to prove that the corona is at least partly due to the earth's atmosphere or mere optical effect. But, as it is, I think this radiation clearly proves that the corona cannot be due to the direct action of the light and heat of the sun on any surrounding matter. In fact, I cannot conceive an atmosphere the character of which varies in a radial manner, however rapidly either its nature or density may vary with the distance from the surface of the sun. If, instead of an atmosphere, we try to conceive a ring of meteors, still the radial gaps so clearly marked on the photographs present insurmountable difficulty. This, moreover, is impossible on other grounds. It is impossible that there can be an almost homogeneous mass of meteoric matter circulating round the sun in the form of an outer sphere, and if it circulated in the ecliptic or any other plane, it would present the appearance of Saturn's belt, whereas the corona appears altogether different from this, and cannot possibly be a film of light in any plane but that of the sun's limb.

Nor can these radial rifts be of the nature of shadow. For the shadow which anything like a sun spot would produce in a misty atmosphere must be conical, the vertex of the cone being outwards, so that the edges of the shadow would approach each other instead of receding as they do. Moreover, such a shadow would still be seen through a great extent of illuminated solar atmosphere, and therefore be only partial or faint, whereas the rifts are so dark and definite as to imply a total absence of coronal light; this must be the case unless the rifts or gaps in the spherical envelope extended right across the sphere from front to back, and we know that there is no obstruction on the surface of the sun that could cast such an extensive shadow.

What, then, does this radiated appearance show the corona to be? I think that it proves that the corona is an emission either of illuminated matter or of an action illuminating matter,

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* I have taken the liberty of italicising those expressions which seem to me of vital importance to the argument in these quotations.

* Has this yet been established?—ED.

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such as electricity, driven off unequally from parts of the sun's surface, and in directions radiating from points either on or beneath the surface.[*] If only a small portion of the sun's surface, such as that covered by sun spots, sent out such streamers, the appearance might exactly coincide with that of the corona; for these streamers, when seen projected on to the plane of the sun's limb, might in some places appear to overlap so as to form a continuous corona, whereas in others they might appear to be separated by gaps.

So far as the appearance is concerned, it would be the same whether the emission consisted of matter or was electricity; there are, however, other indications of its being of the latter kind.

The action which the sun exerts on terrestrial magnetism shows it to be in an electric state, and the observations of Stewart and others have established a connection between the variations in its electric condition and the changes in the sun-spots and red flames, and the observations on the recent eclipse have connected the red flames with the brighter parts of the corona. Here then we have a distinct and independent reason for assuming that the electric condition of the sun's surface is partial and unequal, and for connecting the corona with this electricity.

As I have already ventured to explain the solar corona, as well as comet's tails and the aurora, to be a kind of electric brush, I now offer these remarks on the radial appearance of the corona in confirmation of my views.

Owens College, May 8 OSBORNE REYNOLDS

### A few more Words on Daylight Auroras

IN NATURE for December 29, 1870, there is a letter from Dr. G. F. Burder in reply to a previous correspondent,[†] who had sent a description, with an illustration, of a Daylight Aurora observed by him, wherein he made the following statement:—

But auroral arcs, as far as I know, never appear in the east, and the conclusion, therefore, is unavoidable, that the object observed was nothing more than a remarkably symmetrical form of cirrus cloud."

He then states his convictions that all records of so-called daylight auroras are "errors of observation."

As assertions like these might have an undue influence on the minds of those who read my letter on "Aurora by Daylight" (NATURE, May 4, 1871), I am induced to say a few more words on this subject, especially to prove the fallacy of such reasoning.

It is well known that the aurora borealis assumes innumerable shapes; some of the most remarkable were given by me some time ago in these pages;[‡] and that they appear, at times, actually in the *east*, but more often in the north, north-west, and stretching to the south. A writer of some excellence in the last century[§] says:—

"Sometimes the aurora appears like arches, nearly in the form of a rainbow, reaching from one point of the horizon to another. The arches always cross the meridian at right angles, *tending to the east and west point of the compass*."

The correspondent whom Dr. Burder is so hard upon, most probably saw the arc in a position nearer to this, than directly *facing* him, with his back to the *west*, and as the illustration sent by him shows only a segment of the arc, I am inclined to think that the extremities were nearly in the east and west.[‖] The "cirrus cloud" hypothesis is simply untenable, when it is known positively that on several occasions the aurora was seen against an azure background, with no form of cloud in the field of view, as for instance that mentioned by me, where a faint arc was seen before sunset in the east (possibly N.E.) against a cloudless sky.[¶] His other assertion, from which I must dissent, was: "A comparison of the auroral light with the light of other objects whose visibility can be more easily measured, tends strongly to confirm the view I have advanced." He then instances the invisibility of Donati's comet by daylight. He might have instanced the invisibility of the stars, although they can be seen in broad daylight when the observer is placed at the bottom of a deep pit; but this need not be done, for daylight does not always mean bright sunshine; and with diffused light, Venus is often seen before the sun has actually gone below the horizon. This, so far, may appear mere assertion, but the following will, I hope, be sufficient to show that the view he holds requires some kind of modification.

"A.D. 678.—This year the star (called a comet) appeared in August and shone like a sunbeam every morning for three months" (Anglo-Sax. Chron.).[*] By every morning I take to mean daylight, because in these months the mornings would invariably be very light, especially the few moments before the comet actually disappeared.

With regard to all the record of daylight auroras being mere "errors of observation," I am sure no one will continue to entertain such an opinion after carefully examining all that has been said upon the subject in these pages. As it may be useful to those who are interested in this question, I have made a summary of all the daylight auroras recorded in this and other publications, which do not admit of doubt.

A.D. 1122. A phenomenon appeared like a great and broad fire, and lasted till it was quite light. (Anglo-Sax. Chron.)

A.D. 1467. A *most probable day aurora*, described as "horsemen and men in armour rushing through the air."[†] (Ingulf. Second Cont.)

A.D. 1788. May 5 at 11 A.M. an auroral display seen, consisting of "whitish rays ascending from every part of horizon." Observed by "three different people." (Trans. Royal I. Academy for 1788, quoted by Rev. T. W. Webb in NATURE, May 11, 1871.

A.D. 1827. "Aurora Borealis seen in the day-time at Canonmills" at 4.30 P.M. Described in "Jameson's Journal" and NATURE for May 4, 1871. (Arcana of Science and Art for 1828.)

A.D. 1849.—In September an aurora seen, consisting of "three slightly diverging beams of light on the eastern horizon. One might have taken them for beams from a setting sun . . . had it not been that they did not emanate exactly from the spot where the sun had set; that they had an evident motion to the southard, and that two of them extended to the zenith, and finally down to the eastern horizon."[‡] (Mr. J. Langton, in NATURE, April 27, 1871.)

A.D. 1870.—September 4, about 4.30 P.M., an aurora observed "in the form of thin reddish streaks." ("S. B." in NATURE, October 13, 1870.)

A.D. 1870.—October 25, at 4.30 P.M., a brilliant aurora seen in the *east*, and fully described with illustration of it. (NATURE, December 8, 1870.)

A.D. 1870.—December. A *probable* auroral display, which was observed a "little before sunset," and developed as the evening advanced into a brilliant aurora borealis. The day phenomenon, however, not sufficiently described to make the record trustworthy. (J. Langton, in NATURE, April 27.)

A.D. 1871.—April 10, about 4.30 P.M., a whitish arc seen, almost east, against a cloudless azure sky. On the previous night there was a magnificent aurora borealis.[§] (Mentioned by me in NATURE, May 4.)

These form the whole of the most reliable records, which are certainly few, for the period embraced between the earliest and present date; but I am inclined to believe that the occurrence of daylight auroras is not so rare as is here shown, but that they have been seen and actually recorded in the works which I have here and elsewhere quoted, but for the want of the statement of the time of day or night, one cannot tell to which the appearance belongs. Often a display which can be said to have been seen in the night, might as easily be said to belong to the day, so far as the actual wording of the record goes. It will follow from this that the scanty records we have of daylight auroras referred to phenomena of extraordinary magnitude and magnificence.

JOHN JEREMIAH

### The Conservation of Force

I HAVE been endeavouring to understand what is meant by the Conservation of Force; and as it is one of the most interesting subjects I have studied, I send you the result of my labours.

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[*] We are of opinion that there is still another explanation.—Ed.

[†] NATURE, Dec. 8, 1871. [‡] Ibid, Dec. 29, 1871.

[§] "Compendious System of Astronomy," by Margaret Bryan. London: 1797, p. 132.

[‖] For evidences of night auroras being seen in the east, consult the letters in NATURE for 1870.

[¶] NATURE, May 4, 1871, p. 8.

[*] This is confirmed in Beda, Flor. of Wor. (under A.D. 677) and Chronicum Scotorum (under A.D. 673 in error for 677).

[†] See also Pliny, Bk. II. c. lvii.

[‡] This very singularly explains the following passage in Pliny:—"Round about the sun there was seen an arch when Lu. Opinius and Q. Fabius were consuls" (about B.C. 123). This was not an ordinary halo, for he says further:—"and a *circle* when L. Porcius and M. Acilius were consuls." (Bk. ii. c. xxix.)

[§] It appears curious that the majority of the displays occurred at or about 4.30 P.M., in the autumn, winter, and spring months. Cases of magnetic disturbances during this hour are not rare, accompanying the aurora. It may prove of some value to note this.

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